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A Review of Statistical Ideas Relevant to Intercropping Research

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SUMMARY

The paper summarizes the present state of statistical methods in agricultural research into intercropping, i.e. the growing of two or more crops together. Designs that have been used in intercropping experiments are reviewed. Important statistical considerations for the design of intercropping experiments are discussed and suggestions for improved design given. Available methods of analysis are illustrated with numerical examples and the advantages of each are discussed. Finally, possibilities for further statistical research are outlined.

Keywords: INTERCROPPING; AGRICULTURAL EXPERIMENTS; REPLACEMENT SERIES; FACTORIAL DESIGNS; SYSTEMATIC DESIGNS; BIVARIATE ANALYSIS; LAND EQUIVALENT RATIO; COMPETITION INDEX; STANDARDIZATION; STABILITY; SPATIAL ARRANGEMENT; GENOTYPE SELECTION

1. INTRODUCTION

1.1. *Agricultural Background*

THE semi-arid tropics are characterized by a dry season of between five and ten months and by an annual rainfall of 500–1500 mm. The growing season is short, ranging from 80 to 200 days. The farmer suffers many problems which his counterpart in a temperate climate does not: rainfall varies unpredictably both between and within years, the soil quickly loses its productivity in tropical storms, while the insects, disease and weeds which thrive in the high temperatures cause heavy loss of crops. Ninety-five per cent of farmers manage less than two hectares of land and farming is primitive, with human labour and only simple tools. Traditional cropping systems thus reflect climatic variability and traditional practices as well as economic trends and availability of markets. Disease-resistant varieties are largely unavailable to the small-scale farmer, and fertilizers and pesticides are expensive. The most common system is intercropping: by mixing crops the farmer can have a longer cropping season, longer protection for the soil from sun, rain and weeds, and more constant use of labour; he can also grow a greater variety of crops and can achieve greater yields or profit. The advantages of changing to any other system must be considerable before any farmer will deviate from traditional practices.

Much of the work in tropical agricultural research concentrates on developing recommendations for growing sole crops, with the aim of increasing mechanization. However, the farmer still mixes his crops and is not easily persuaded, rightly or wrongly, to change his farming practices. Although mechanization is being introduced, it will not be of great advantage until machine maintenance is available: in villages where machinery has been tried, it is not uncommon to see a tractor abandoned because of its failure to work. The use of standard machinery is difficult when species are mixed. Even for sole-crop agriculture it has been found in some countries that, after the introduction of mechanization, farmers have resumed using oxen and plough. The persistence of traditional farming and the appearance of experimental results indicating yield advantages of mixing crops imply that improved intercropping research should benefit the small-scale tropical farmers.

The complexities of intercropping become apparent when considering the continents where this system is more commonly practised: Africa, India, Asia and South and Central America. Climate and geography vary widely both between and within countries. Consequently one particular intercropping mixture can produce very different results in different areas, and many different mixtures may have to be considered.

The importance of intercropping is indicated (Francis *et al.*, 1977) by proportions of crop areas that are mixed: 98 per cent of cowpea in Africa (Arnon, 1972), 90 per cent of beans in Colombia, 73 per cent of beans in Guatemala (Guttierrez *et al.*, 1975), 80 per cent of beans in Brazil (IICA, 1969) and 60 per cent of maize in Latin America (Francis *et al.*, 1977). Comparisons with sole cropping have shown yield advantages for many mixtures, e.g. beans and maize (Willey and Osiru, 1972), millet and sorghum (Andrews, 1972), potato and barley (Jain, 1978) and sugarcane and maize (Pillay and Mamet, 1978).

Many intercrop combinations have been used. The most common is a cereal–legume mixture, the more usual cereals being maize, pearl millet and sorghum, with legumes including chickpea, cowpea, pigeonpea and many bean crops. Other crops have included sugarcane, coconut, potatoes, cassava, groundnuts and cotton.

1.2. *Literature on Intercropping*

Most early papers on intercropping study competition more from a botanist's point of view than from an agronomist's, with competition interpreted in terms of the separate crops' yields rather than combined yields. Some of the more important of these papers are by Donald (1946, 1961), Aspinall (1960), Black (1960), de Wit (1960) and Harper (1961). Complex statistical analyses for competition experiments have been developed by Williams (1962), McGilchrist (1965) and McGilchrist and Trenbath (1971). A short review of various aspects of competition experiments is given by Mead (1979).

Aiyer (1949) gives a comprehensive review of early work; more recent reviews are by Dalrymple (1971), Crookston (1976) and Willey (1979a, b). Many other references are in the annual reports of international research centres which have regular programmes of intercropping work: Institute for Agricultural Research, India; The International Crops Research Institute for the Semi-Arid Tropics (ICRISAT), India; The International Rice Research Institute, Philippines; Centro Internacional de Agricultura Tropical, Colombia; International Institute of Tropical Agriculture, Nigeria. In addition there have been major conferences on intercropping: the Multiple Cropping Symposium at the American Society of Agronomy Annual Meeting, Tennessee, 1975; the Symposium on Intercropping in Semi-Arid Areas, Tanzania, 1976; the Symposium on Intercropping with Cassava, Philippines, 1978; the International Workshop on Intercropping, ICRISAT, India, 1979.

Much research has concentrated on yield advantage, but other advantages include the better control of weeds, pests and disease, and better use of light and nutrient resources (Willey, 1979a; Belshaw and Hall, 1972).

Little of the intercropping literature includes comment on experimental design for intercropping research or on statistical analyses appropriate for the data involving more than one crop. Here, methods of design and analysis of intercropping experiments are reviewed and suggestions for extending these techniques are given.

1.3. *Terminology*

Terms used to describe the growth of more than one crop together include intercropping, mixed cropping and relay cropping. In this review, "intercropping" refers to the mixture of more than one crop simultaneously for some, if not all, of their life-cycles and irrespective of their spatial arrangement. "Mixed cropping" has often been used in the same sense as intercropping and so is not used here. "Relay cropping" refers to a system where different crops are sown on the same land but their growing periods overlap only briefly, or not at all, and experiments on such systems are not included in this review.

It is important to distinguish between intercropping experiments and competition experiments with mixtures. In intercropping, the objectives are essentially agronomic, to find the best way of growing an intercrop combination; an intercropping experiment will normally include a possibly large number of experimental treatments for a single-crop mixture. In competition experiments the objectives are more purely biological, to understand the mechanism of competition by examining which species or genotypes show competitive benefit when grown in mixtures; a competition experiment will normally include mixtures and pure stands of many species or genotypes, grown under one or a few environments or treatments. Recent papers by Federer and his colleagues (Federer, 1979; Federer, Hedayat, Lowe and Raghavarao, 1976) have been essentially concerned with competition experiments rather than intercropping and they will not be reviewed here.

Other terms used frequently here are "sole crop" when a single crop is grown, "component crop" for one of the two or more crops grown together, and "intercrop" for the particular crop mixture. A common form of experiment in competition studies is the "replacement-series experiment" in which the experimental treatments include both of the pure stands or sole crops, and various mixtures consisting of a proportion, p, of the pure stand of one component crop, and a proportion $(1-p)$ of the pure stand of the other component crop.

2. THE DESIGN OF EXPERIMENTS

2.1. *The Present Situation*

To assess the experimental designs currently used in intercropping, we examined several recent papers including those of the International Workshop on Intercropping at ICRISAT in January 1979, and others from the *Journal of Experimental Agriculture*. The experiments are diverse, covering many different aspects of intercropping, but over 90 per cent of them have a simple treatment structure with one or two treatment factors. Frequently, when there are two factors, one is a set of different crop combinations, for each of which only one other factor is thus investigated. Almost all the experiments have four or more replicates. Where two, or occasionally three, factors are included, about three-quarters of the experiments have split-plot designs. Sometimes the introduction of a second treatment factor is avoided by setting up separate experiments, each at one level of the additional factor. Apart from a few systematic designs and two criss-cross designs, all experiments used a simple randomised block or split-plot designs.

In most of the experiments much of the area is occupied by sole crops, in order to provide a comparison for the intercropping yields; but there seems to be a strong tendency to continue asking whether mixed cropping is better than sole cropping at the expense of investigating agronomic problems of intercropping.

In the experiments reviewed, there were many comparing a wide range of different intercrop mixtures. The ideology of this group of experiments appears to be to see what crops can usefully be mixed, the intercrops being diverse because peasant farmers use many combinations. It might be better to survey farmers' practice and to base intercropping research programmes on the most popular combinations rather than replicate their long years of experience.

It is obviously dangerous for statisticians to lay down research priorities, and we have therefore relied substantially on papers by Willey and his colleagues. It seems clear that the main priorities are

(a) identifying suitable crop combinations (e.g. Okigbo, 1974),

(b) accurately assessing yield advantages of intercropping compared with sole cropping (Willey, 1979a),

(c) identifying suitable genotypes for intercropping (Francis *et al*., 1975),

(d) investigating optimal spatial arrangement and plant populations (Willey, 1979b),

(e) estimating the legume contribution to the yield of the cereal (references in Section 1), and

(f) investigating the stability of intercropping in the sense of consistency of yield, using experiments at different sites and with various nutrients and management systems (Jodha, 1976; Norman, 1976).

The statistical problems of intercropping research are many and the opportunities for interesting statistical work are exciting. There is much ignorance about appropriate levels of the various factors involved, e.g. optimal population densities for some component crops may be higher (plants per plot) than the optimal densities for the sole crops. The resources spent on intercropping research in the next decade may be large so that results may be expected rapidly. Statistical theory is much further advanced than when research into sole cropping was at a similar stage. The challenge for statisticians is to use the greater theoretical knowledge to improve the efficiency of intercropping research.

The statistical problems of design are not very different from those for sole cropping. There are many factors of potential interest—more than in sole cropping—mainly due to the increased complexity of the spatial arrangement of an intercrop. Most experiments will be on land recently converted to experimental use and not very homogeneous—there is some evidence that variability is greater in intercropping than in sole cropping, whether the variation is of a single component-crop yield or of a combined crop yield index.

As regards experimental design there are three topics on which experimenters need much more guidance and on which the general statistical philosophy is quite clear; these, discussed briefly in Section 2.2, are the advantages of factorial structures, the choice of levels for quantitative factors, and the disadvantages of split-plot designs. In addition to these general topics there are three others of particular relevance to intercropping: the use of sole-crop treatments within intercropping experiments (Section 2.3), genotype selection (Section 2.4) and spacing and population-density effects (Section 2.5). Finally, the use of systematic designs, which have become popular in intercropping research, is discussed (Section 2.6).

2.2. *General Statistical Theory Needed by Experimenters*

A fundamental tenet of the statistical theory of experimental design is that factorial experiments are more efficient than experiments on one factor at a time. Theoretically the advantages of hidden replication are overwhelming; however, in practice we have found experimenters extremely reluctant to adopt even moderately saturated designs. It is particularly important to gain the benefits of factorial experiments during the initial research into intercropping by investigating the extent to which pairs of factors may interact; later, more detailed experiments about agronomically optimal treatments can then be designed efficiently. However, we must recognize that many experimenters naturally think, at an early stage of an experimental programme, that taking each factor in turn is logical, simply because they do not understand the effects of other factors: consequently they will need much persuasion to accept designs with many factors. To illustrate the kind of design we are considering, Fig. 1 shows one of three replicates of a confounded $2 \times 2 \times 3$ growth analysis experiment at ICRISAT, for sorghum (S) and pigeonpea (P), requiring large plots for many harvests. The design originally proposed had only a subset of the treatments finally used, together with two extra sole-crop treatments, in four blocks of eight mixed-crop treatments plus four sole-crop treatments. To achieve the design of Fig. 1 required several days of persuasion.

The main argument against the use of more treatment factors in intercropping research occurs frequently in this review and arises because most intercropping experimentation is in the tropics where the experimental material is more heterogeneous than in the well-established research institutes of temperate climates. If very small blocks are needed because of heterogeneity then some of the advantage of factorial structure may be lost.

Much modern design theory concerns the choice of levels of a quantitative factor, and is very general; it involves criteria that are unlikely to have much immediate appeal to the agricultural experimenter. Nevertheless some conclusions emerge with sufficient clarity from the theory, and from the few results for particular questions, for them to become fundamental principles of the

$S_1P_2A_1$	$S_2P_1A_2$
$S_2P_3A_2$	S
P	$S_1P_1A_1$
$S_2P_2A_2$	$S_1P_3A_1$

$S_2P_1A_1$	$S_1P_3A_2$
P	$S_1P_1A_2$
$S_1P_2A_2$	$S_2P_2A_1$
$S_2P_3A_1$	S

FIG. 1. One replicate of a $3 \times 2 \times 2$ factorial experiment with 2 sole-crop treatments, arranged in two blocks of 8 plots. S_1, S_2 are 2 sorghum population densities; P_1, P_2, P_3 are 3 pigeonpea population densities; A_1, A_2 are 2 proportions of sorghum : pigeonpea; S and P are sole-crop sorghum and sole-crop pigeonpea.

design of practical experiments. The primary conclusion concerns the number of levels. If a response function is to be fitted to the observed yields then the factor needs as many levels as there are parameters in the function. To examine the lack of fit of the function, at least one further level is needed. Effectively this means that the use of more than four levels of a factor cannot easily be justified; frequently three levels will suffice. We have found this principle, though initially surprising to experimenters, to be quite rapidly accepted by them. However, the logic is not so easily accepted when applied to the number of harvests in growth analysis experiments. It may be objected that, if the experimenter does not know the form of the response, or growth, function, he should use more levels, but we believe that this does not justify, say, 12 or 15 sampling times in a growth analysis experiment. To discriminate between several response functions requires more levels, but discrimination spread over many levels is inefficient and an absolute upper limit of six could well be appropriate. General principles suggest that the levels should be spread over as wide a range as is practicable. It is less clear where intermediate levels should be taken, though equal spacing seems never to be markedly sub-optimal.

Many recent experiments on intercropping use split-plot designs, not generally for any strong practical reasons of treatment application but for simplicity of plot allocation of treatments. The theoretical disadvantage of having multiple standard errors and levels of precision usually far outweighs the advantages of split-plot experiments, except where the split-plot design is inevitable for practical reasons. In intercropping, with several factors, many comparisons between particular combinations of levels are likely to be important. The split-plot design involves different comparisons having different precisions in a much more complex pattern than is understood by the experimenter who has read that "split-plot designs improve the precision of estimation of interactions." Unless he has considered the detailed implications, he would do better to avoid split-plot designs, except through practical necessity. Unwittingly, perhaps, many statisticians have presented the ideas of split-plot designs too simply, with insufficient concern to ensure that experimental designs are efficient. One of us has had to suffer hearing statisticians meekly presenting the experimenter's argument that split-plot designs make it easier to show experiments to visitors!

2.3. *The Use of Sole-Crop Treatments in Intercropping Experiments*

In Section 3 it is argued that comparison of different intercropping treatments does not require sole-crop yields for each component crop to be estimated for each experimental treatment. Also, sole-crop yields required only as standardizing measures for combining the yields of the two component crops need not be analysed with the yields for intercrop treatments.

Sole-crop plots needs then not be included within the experiment: areas of sole crops grown around or alongside the experimental intercrop plots allow the experimental treatments to be compared within a more compact area but still provide good estimates of sole-crop yields for standardization (Mead and Stern, 1980).

More generally, intercropping experiments designed to assess whether a particular intercrop yields an advantage compared with the corresponding sole crops must be distinguished from those to compare different methods of growing a particular intercrop. Of course, some experiments have objectives not clearly in one group or the other. However, many experiments include large numbers of sole-crop plots for no very good reason. The problem is similar to that of including control treatments in traditional sole-cropping experiments but is exaggerated because of having two or more potential sole-crop treatments for each intercropping treatment.

2.4. *Genotype-Selection Programme*

A major part of intercropping research concerns the selection of suitable genotypes. Theory for the selection for a single crop indicates that similar resources and intensities of selection should be used at each stage of the selection procedure (Finney, 1958). There is often no clear reason why the appropriate genotypes for intercropping should be the same as those for sole cropping. This can be illustrated by results for the important sorghum/pigeonpea intercrop. Here, pigeonpea is the dominated crop growing slowly until the sorghum is harvested and rapidly thereafter. Table 1 shows the mean yields of seventeen pigeonpea genotypes, listed in sole-crop order, grown both as sole crops and, with a standard sorghum genotype, in an intercrop. Clearly the means of different genotypes in the sole crops and in intercrops are related, but not strongly (these data are displayed further in Section 3.4). Other sets of data show both stronger and weaker relationships.

TABLE 1
Pigeonpea genotypes intercropped with sorghum mean yields for four replicates (kg/ha)

Sole-crop Pigeonpea	*Intercrop Pigeonpea*	*Intercrop Sorghum*
1699	850	3804
1525	842	3931
1428	740	3640
1407	815	3630
1389	757	3386
1376	885	3344
1323	799	3899
1296	619	3381
1264	585	3973
1226	619	3757
1222	512	3232
1185	463	3500
1169	503	3323
1148	661	3930
1106	718	3198
1063	530	3645
1058	720	3677

To consider selection for intercropping, suppose that three stages involving (a) several hundred, (b) about 40, (c) about 6 genotypes are to be considered. Then selection of suitable genotypes for intercropping could involve each of (a), (b), (c) for one crop with each of (a), (b), (c) or a single genotype for the second crop. The sheer size of (a) × (a) and of some other combinations makes them impossible and the experimenter has to consider for each particular crop combination which selection experiments are sensible. A possible philosophy for the sorghum/pigeonpea intercrop is outlined below.

Because pigeonpea is very much the dominated crop the ideal sorghum genotype is likely to be similar to the ideal sole-crop sorghum genotype. The major screening effort should therefore be on pigeonpea. Only a single sorghum genotype may be needed in the first screening stage involving several hundred pigeonpea genotypes. If the screening is solely concerned with intercropping there seems to be no reason for including sole-crop pigeonpea plots. The next stage, with 40 pigeonpea genotypes should again include only a single sorghum genotype but, for assessing the biological advantage of intercropping, all pigeonpea genotypes should probably be included as sole crops, and ideally a nutrient factor also. The last stage might suitably include 4 to 8 pigeonpea genotypes with similarly many sorghum genotypes to compare the performance of particular combinations. Again sole-crop plots could be dispensed with.

2.5. *Spacing and Population-Density Experiments*

Investigating spacing effects for an intercrop is more complex than for a sole crop because there are five factors involved: the density and spatial arrangement of each component crop, and the relative arrangement (or intimacy) of the two crops. To our knowledge, no intercropping experiment has involved independent variation of as many as three spacing factors, and there is obviously a danger of misinterpreting effects when so many factors are aliased. For example, in a typical replacement-series experiment (Fig. 2), a constant between-row spacing is frequently used and in the treatment sequence A-B-C-D the two component-crop densities change monotonically, in opposite directions, and spatial arrangement (ratio of between-row to within-row distance) of each crop also changes. Only the intimacy is invariant, as the rows of the two different crops are always as closely intermingled as is possible.

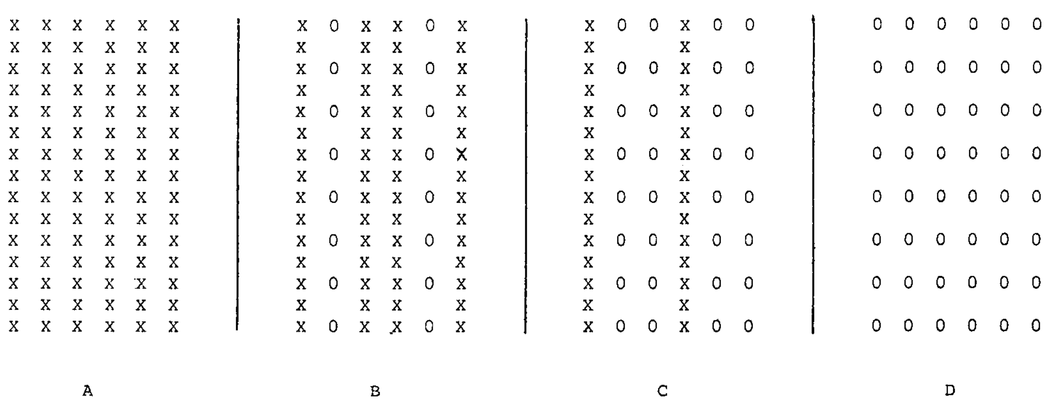

FIG. 2. Four treatments (A, B, C, D) for a replacement-series experiment in which the overall density remains unchanged, assuming a ratio X:0 = 2:1.

Spacing experiments based on a Nelder (1962) fan design have been considered by Huxley and Maingu (1978) and Wahua and Miller (1978). In both experiments the ratio of the densities of the two component crops was held constant in each segment of the fan, while the overall density was systematically varied. Huxley and Maingu also varied the density ratio between segments of a fan. These designs are effective at isolating one, or two, spacing factors; they do not provide information on the effects and interactions of several spatial factors.

A major problem of spacing experiments is the difficulty in disentangling the effects of the five factors: no design has yet been constructed which allows all five to be varied independently. Fig. 3 shows how the two spatial arrangement factors and the intimacy can be varied separately, with the density per unit area constant for each crop. In this Figure, (a), (c) and (e) show combinations of spatial patterns for the two crops arranged relatively more intimately, whilst (b), (d) and (f) show the same patterns arranged less intimately. There is a yet more intimate version of (a) but not of (c) or (e). In (a) and (b) the patterns for each separate crop are nearly square, whereas in (c) and (d) the plants of each individual crop are much closer within the rows. In (e) and (f) one crop has the squarer pattern and the other the less square pattern and there are obviously two further arrangements completing the set of 2^3.

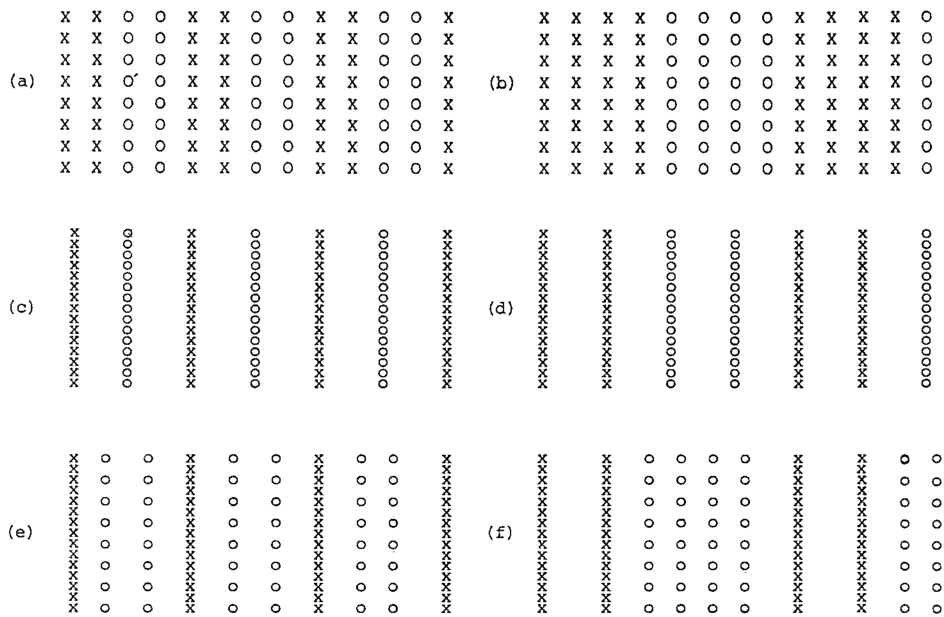

FIG. 3. Variation of intimacy and spatial arrangement of each crop for a two-crop mixture.

The densities of the two crops could be varied simultaneously for the whole set of 2^3 spatial arrangement treatments by changing the within- and between-row distances by the same factor. However, it is hard to see how to vary the two densities independently without altering the structure of the 2^3 treatments. Any further geometrical solutions would be welcomed.

2.6. *Systematic Designs*

It is almost universally accepted that, in an experiment to compare a clearly specified set of treatments, it is good statistical practice to randomize the allocation of treatments to units, within a prescribed set of restrictions. Nevertheless, systematic experimental designs have been found useful for two main types of agricultural experiment: systematic log–dose trials of herbicides in which the herbicide concentration is reduced by a constant proportion from row to adjacent row (Thompson and Wheatley, 1977), and designs to investigate spacing effects (Nelder, 1962; Bleasdale, 1967) where the crop density or the plant arrangement, or both, change monotonically across each large experimental plot. Systematic designs are also being used in intercropping experiments (Wahua and Miller, 1978; Huxley and Maingu, 1978; Willey, 1979b).

Experimenters, and some statisticians, advocate the use of these designs because, by avoiding the loss of large proportions of the experimental area as guard areas, the designs use the experimental area efficiently. It seems important that the arguments for and against the use of systematic designs should be considered now in the hope that statisticians might achieve an agreed policy on the use of such designs. This discussion will be restricted to systematic designs in which levels of one or more experimental factors increase systematically across each large plot, such large plots being replicated and the direction of the systematic change being randomized for each large plot; other additional experimental factor levels may be applied (randomly allocated) to the large plots.

The primary statistical argument for randomized rather than systematic designs is that randomization provides a justification for the analysis. In some circumstances an experimenter may justifiably believe that each block (area of land) in his experiment is sufficiently homogeneous for the plot variation to be considered as almost entirely due to plant variability, and that the random selection of plant material suffices to justify an analysis of variance. This argument has some validity, since plot variability, when compared over several experiments, often seems closely related to the number of plants per plot, but should be viewed with suspicion where, as in the tropics, there is less prior information on the relative contribution of plants and soil to the plot variation.

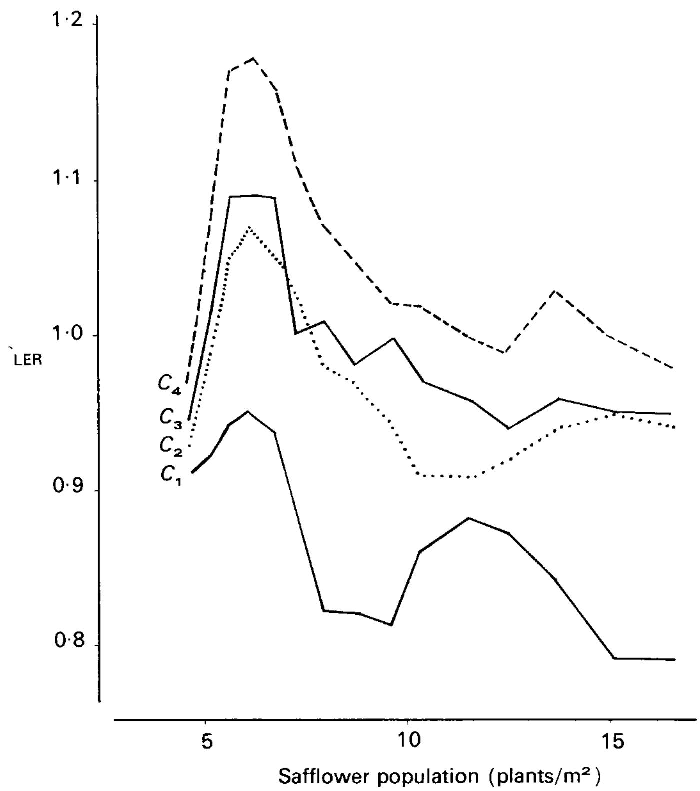

FIG. 4. Effects of population densities on LER for safflower (systematic variation) and chickpea (randomized variation).

A major argument in favour of the systematic designs is that the first stage of the analysis of data from an experiment will frequently involve a simpler approach than analysis of variance. To see why this should be so, consider the experimental results shown in Fig. 4. The experiment included fifteen within-row densities for safflower in a systematic arrangement with the density increasing by 10 per cent from row to row, and four chickpea population densities, C1, C2, C3 and C4, allocated to main plots in four randomized blocks. The graphical presentation of a combined yield index (see Section 3.1) displays the results adequately. A more formal analysis would involve fitting an appropriate response function to the results for the 15 safflower densities for each main plot. Parameters of the fitted response function could be compared between chickpea densities by a standard analysis of variance, since the chickpea densities are allocated randomly to main plots. Thus, if the analysis of the results of the experiment is to consist of fitting a response curve for each main plot (as a function of the factor varied systematically within the main plot) followed by an analysis of the fitted response curves, the systematic design may be an appropriate one.

Randomization of spacing treatments within blocks, on average, eliminates the effects of trends along the blocks, although individual plots should ideally be laid out to run along trends not across them. If, in a systematic design, the spacing treatments vary along a trend then the trend reveals itself as large "random" variation between the response curves fitted for different replicates of the systematic large plots; in this case the systematic plots should ideally be laid out to run across trends.

Another important consideration is the scale of the heterogeneity within the overall experimental area, that is, the size of area that may be regarded as homogeneous. This is particularly important in intercropping experiments because these are mostly in experimental fields which have been used as such for only a short time. In a typical randomized-plot design with sorghum and pigeonpea at ICRISAT, the standard practice would be to use a plot size of 4·5 m × 9 m, of which 2·7 m × 7 m would be harvested. In a corresponding systematic design with twelve densities of pigeonpea (or sorghum) the complete plot would be 18 m × 9 m, of which 16·2 m × 7 m would be harvested. To compare the effects of different densities, consider two alternative designs—a systematic design or randomized plots—for four density treatments, each design having additional treatments and also replication. If the scale of heterogeneity is larger than a systematic plot, heterogeneity will have similar effects on both designs, hopefully allowed for by blocking. When the scale of heterogeneity is between the sizes of the randomized plot and the systematic plot, there will be the possibility of bias in estimating the response pattern for a single systematic plot, the effect of the bias being to increase the variation of the fitted response curves, between replicate systematic plots. The effect on the randomized plot will be simply an increase of within-block variances. Finally, when the scale of heterogeniety is smaller than the randomized plot the effect becomes effectively random within each systematic plot and will be similar for both designs.

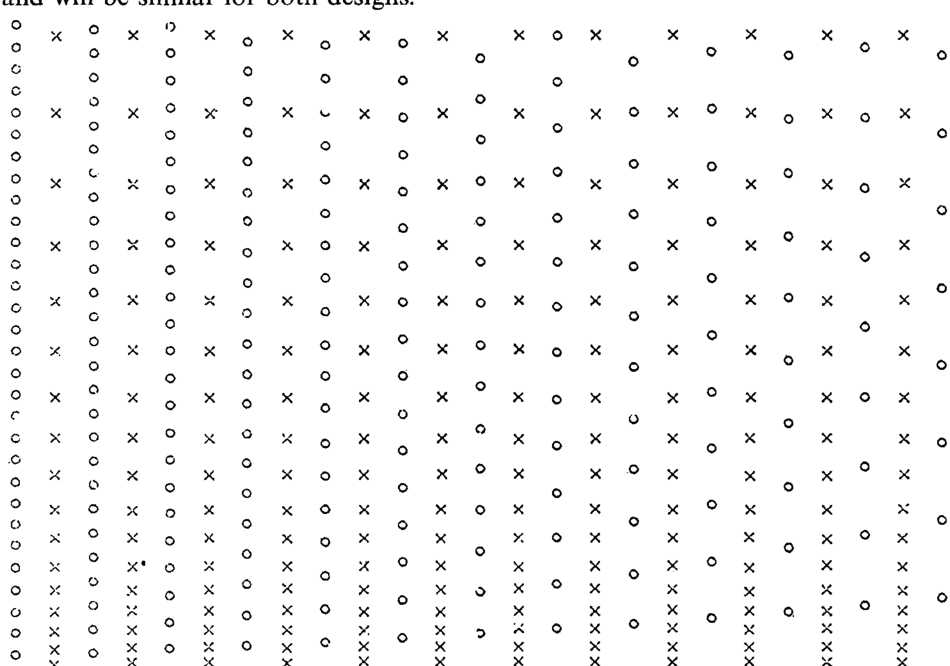

FIG. 5. Two-way systematic spacing design for two crops (× and ○) with the densities varying in perpendicular directions.

These arguments are obviously general and intuitive but may help the experimenter, who, after digesting them and other similarly intuitive arguments about his experimental material, has to choose an experimental design.

We believe that systematic designs deserve a place in intercropping research and we hope that statisticians will recognize that considerable benefits may be gained by using systematic designs of the types discussed here. Their main advantage is clearly that they greatly increase the proportion of the total area which is harvested (in the example given earlier the increase is from 47 to 70 per cent). This advantage is most notable when a very wide range of crop densities is being investigated.

As mentioned in Section 2.5, systematic spacing designs based on fan designs have been tried in intercropping. However, the most appropriate systematic design for intercropping work is probably Bleasdale's (1967) row version of the fan design. The advantages of the row design are that crops will conveniently be grown in parallel rows, and that densities can be varied both along and between rows. An obvious design for investigating independent variation of two crop densities is shown in Fig. 5 (Mead, 1979). Alternatively, one or both densities can be varied along the rows and the intimacy of arrangement and spatial arrangement varied systematically between rows.

We hope that biometricians will look for other systematic designs, and that experiments will be set up to compare the efficiency of systematic and randomized-plot designs. One aspect of systematic designs which must always worry the enthusiast for them is the large number of treatment levels used, which conflicts with the principle of using the minimum necessary number of levels (Section 2.2).

3. ANALYSIS

It is generally accepted that more than one analysis should be applied to intercropping data (Mead and Stern, 1979). As there is no standard method of analysing data from intercrop and sole-crop plots together, it is sensible to have first separate analyses for the sole-plot yields of each crop, for the yields from both the sole and mixed plots of each crop, and possibly for only the mixed-plot yields of each crop. Later an analysis of combined yields of the crops can be considered.

There has been little work on the analysis of such combined yields. In the related subject of competition studies, various indices have been proposed; these are critically reviewed in Section 3.1. One index, the Land Equivalent Ratio, has been used in interpreting intercropping data; two recent developments in its use are discussed in Sections 3.2 and 3.3. Pearce and Gilliver (1978, 1979) have used bivariate analysis of variance as the basis of a method described and discussed in Section 3.4. To illustrate the methods of Sections 3.2, 3.3 and 3.4 we shall use data from a millet/sorghum intercropping experiment at ICRISAT in 1978. The experiment involved four millet genotypes (M_1, M_2, M_3, M_4) and four sorghum genotypes (S_1, S_2, S_3, S_4). The sixteen mixture combinations plus four sole-millet and four sole-sorghum treatments were laid out in a randomized block design with four blocks of twenty-four plots.

Intercropping is often advocated as more stable than sole cropping, in the sense of providing the more consistent returns for the farmer. The idea of stability is poorly developed and the problems of defining stability are briefly discussed in Section 3.5.

3.1. *Indices of Competition and Combined Yield*

Many different indices of combined yield have been suggested. Most were initially used for investigations into competition between genotypes of a particular species, rather than between intercropped species. Further, some indices are restricted to interpreting replacement-series experiments, which have dominated most research into competition.

The three possible patterns of results from a replacement-series experiment are described clearly by Willey (1979a) and are reproduced in Fig. 6. Mutual inhibition (6a) occurs when both

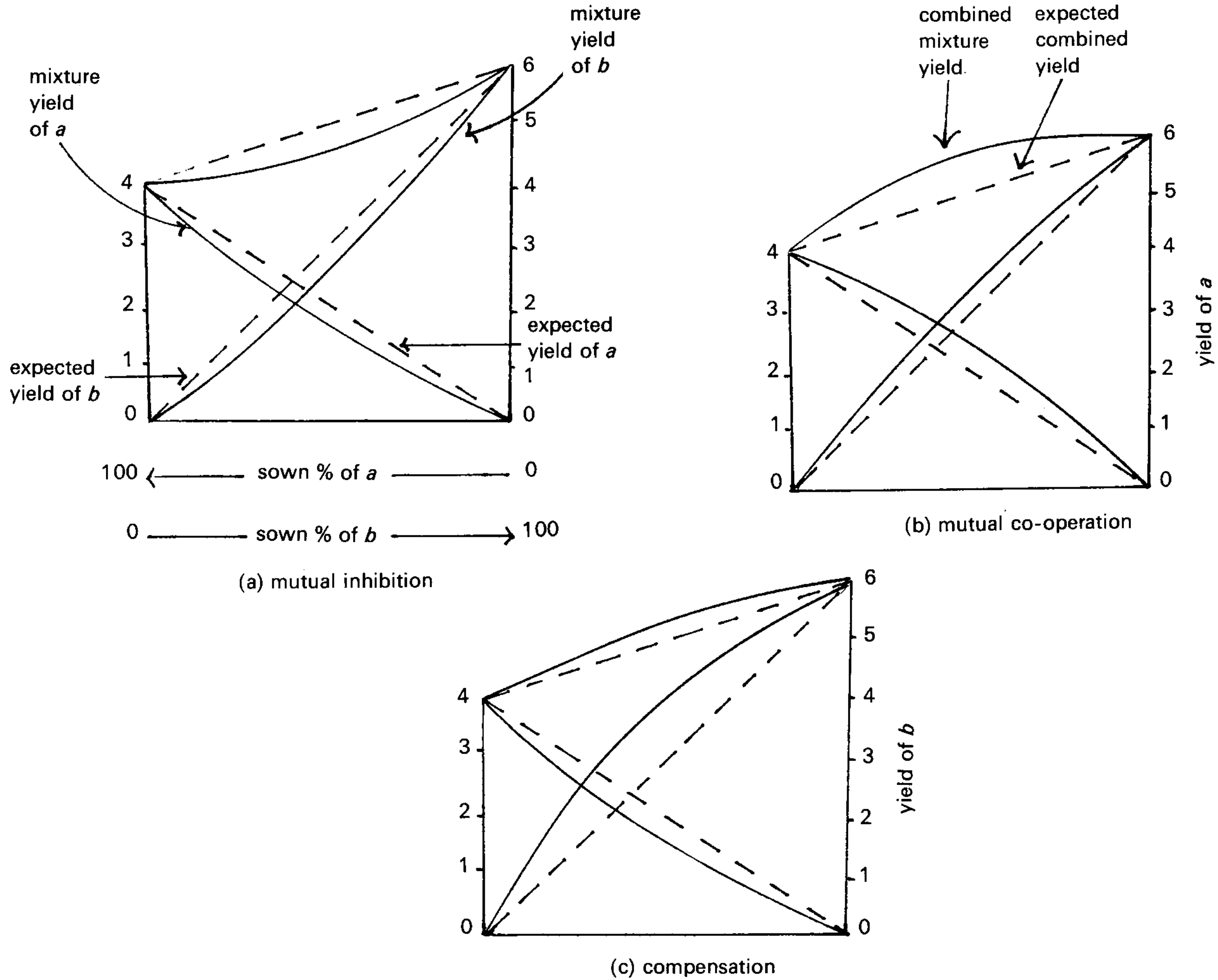

FIG. 6. Competition between species.

species yield less than would be expected from the density of each species if grown separately, mutual co-operation (6b) when both species yield more than expected, and compensation when one yields more than expected and the other less (6c). A disadvantage of this widely-used diagrammatic approach is that, with compensation, the apparent overall advantage may be illusory. The "expected" yields in Fig. 6c are calculated assuming that the area is divided into two equal pure stand areas whereas the actual yields of the two species in a 50 : 50 mixture could have been achieved by dividing the area into two pure stand areas in the proportions π and $(1-\pi)$, where $\pi \neq 0{\cdot}5$. Essentially, when the dominant species, yielding more than expected, gives the greater yield, the diagrammatic comparison of intercrop with sole crops is biased in favour of mixing. Willey (1979a) argues that comparisons of intercrops with sole corps, if based on the sown proportions of the species, are biased in terms of the farmer's practical benefits. He argues that the simple comparison of Fig. 6c is biased towards intercropping if the dominant species gives the greater yield, and towards sole cropping otherwise.

Willey and Osiru (1972) suggested overcoming this bias by defining a measure of the efficiency of an intercrop in terms of the land areas required under sole cropping to give the yields obtained from the component crops. The combined sole-crop yields that could be obtained from these areas would be equated to the combined intercrop yields, and the efficiency of growing a crop mixture assessed by whether the total of the two areas required for sole crops was more or less than the area for the intercrop. Formally, Willey and Osiru defined, as an index of combined yield, the Land Equivalent Ratio (LER)

$$\text{LER} = L_1 + L_2 = \frac{y_1}{s_1} + \frac{y_2}{s_2},$$

where y_i is the yield per unit area of component crop i of an intercrop, s_i is the yield per unit area of the crop grown sole, and L_i is the separate component crop LER. Obviously, the LER is the relative land area required to produce the same yields by sole cropping as are achieved by intercropping. The LER is identical to the Relative Yield Total of de Wit and van den Bergh (1965), who were primarily interested in replacement-series experiments and who developed their index in terms of the proportions of sole-crop yield achieved by intercropping. However, an important property of the LER is that, because it is not restricted to replacement-series experiments, the quantities s_i can be yields for sole crops possibly using different genotypes or treatments from the particular intercropping mixture.

Various other indices of competition or combined yield have been suggested, all of which can be related to the LER components. The Relative Crowding Coefficient, K, was suggested by de Wit (1960) and developed by Hall (1974a, b). A coefficient K_i is defined for each species in a replacement series. If p_1 = sown proportion of species 1, ($p_2 = 1 - p_1$), then

$$K_1 = \frac{y_1(1-p_1)}{(s_1 - y_1)p_1}.$$

A value of K_1 greater than 1 shows that species 1 has yielded more than expected (in terms of sown proportions of the two species). The product

$$K = K_1 K_2 = \frac{L_1 L_2}{(1-L_1)(1-L_2)}$$

is an index of overall yield advantage ($K > 1$) or disadvantage ($K < 1$).

Donald (1963) introduced an equivalence factor for each species in terms of the number of plants required to produce a given yield per unit area when using sole crops (N'_1 = number of plants required) or mixed crops (N_1 = number of plants of species 1 in mixture). His Competition Index is then defined as

$$\left(\frac{N'_1 - N_1}{N_1}\right)\left(\frac{N'_2 - N_2}{N_2}\right).$$

By definition, the LERs for the two component crops in Donald's situation of equal crop yields will each be 1, giving a total LER of 2. In practice Donald's index is difficult to use as it requires that sole crops be grown at a wide range of densities to estimate N'_1, N'_2.

The above indices have all been measures of overall yield advantage or disadvantage. Another aspect of competition is the dominance of one species over another. The Coefficient of Aggresivity, A, was proposed by McGilchrist and Trenbath (1971), who extended work by Williams (1962) and McGilchrist (1965) on analysing replacement series. Again, this coefficient is defined in terms of the difference between the relative yield increase for species 1 as compared to species 2.

$$A = \frac{y_1}{s_1 p_1} - \frac{y_2}{s_2 p_2} = \frac{L_1}{p_1} - \frac{L_2}{p_2}.$$

Willey and Rao (1980) suggested the alternative Competitive Ratio

$$CR = \left(\frac{y_1}{s_1 p_1}\right) \bigg/ \left(\frac{y_2}{s_2 p_2}\right) = \left(\frac{L_1}{L_2}\right)\left(\frac{p_2}{p_1}\right).$$

A difficulty in interpreting any dominance coefficient is that apparent dominance is related to the particular densities used for the sole-crop yields (s_1 and s_2). J. Connolly (personal communication) has shown that dominance patterns can be reversed by changing sole-crop densities.

Willey (1979a) compared the Relative Crowding Coefficient, the LER and the Aggresivity Coefficient using data from an experiment which compared all combinations of four genotypes of pearl millet with four of sorghum in equal proportions. For each combination all the indices picked out the same species as the dominant one, or agreed when neither species was dominant. The Relative Crowding Coefficient and the LER showed the same pattern of yield advantage or disadvantage; the Aggresivity Coefficient, not surprisingly in view of the different underlying philosophy, did not. Unlike the LER, the Relative Crowding Coefficient was not effective in showing the magnitude of yield advantage. Willey therefore argued that the LER was the most useful index, with the additional advantage that it can be defined for any set of intercropping treatments, not just for replacement series.

All these indices have been introduced in the context of a particular crop combination. In intercropping however, different crop combinations may have to be compared. The LER can be used for this, but comparing LERs for different crop combinations is not always straightforward and various other indices of combined yield have been suggested. Most of these involve a simple conversion of yields to cash value, total protein or some other scale. Undoubtedly, economic indices have advantages, but they have the disadvantages that (i) the economic values may change, and (ii) the indices are based on the sown proportions of the crops, not the harvested proportions.

3.2. *Problems of Standardization in Using the Land Equivalent Ratio*

The LER was introduced to measure increased biological efficiency in terms of harvested yields rather than of sown seed. However, two problems arise in the use of the LER to compare different mixtures; these are discussed in this Section and the next. The first problem is that the LER is defined in terms of ratios of yields from crop mixtures to sole-crop yields, and that large values of the LER arise not only when the intercrop yields are large but also when the sole-crop yields are small. This dependence on the success or failure of the sole crop makes the simple LER unreliable for comparing intercropping mixtures. Mead and Willey (1980) suggested that s_1 and s_2 be regarded as standardizing factors and that it may sometimes be more sensible to define s_1 and s_2 as the maximum or average of the sole-crop yields for the set of treatments in the experiments.

The aims of the experiment should determine the method of standardization. Huxley and Maingu (1978) argued that, for an experiment with different plant populations and spacings, all intercrop yields should be compared with the sole crop at the optimum population and spacing, since plant population and spacing are easy and cheap to alter.

Other situations may require the use of different standardizing factors. For example, when a treatment such as herbicide or fertilizer is not constantly available to the farmer, the use of more than one sole-crop yield will indicate the relative advantages of intercropping that can be achieved for the different fertility levels.

An experiment with different genotypes could well be interpreted in more than one way. To determine the highest yielding combination, the intercrop yield could be compared with the highest yielding genotypes of each crop, but to determine the relative biological efficiency of a combination, the comparison would have to be with the sole genotype of that combination.

Besides practical relevance in the choice of standardized yields there are statistical questions of precision. Where there is no obvious practical principle leading to standardization either by a single sole-crop yield or by separate sole-crop yields for different treatments, a method of standardization could reasonably be chosen on the basis of the precision of treatment comparisons. However, there is a further complication, namely whether the standardization yields should be the same for the whole experiment, or should vary between blocks. Fisher (1977, 1979) argued for standardization within each block, to reduce standard errors and also the skewness of the distribution of LERs.

Pantelides (1979) compared six methods of standardization for two genotype-mixture experiments, including the experiment described at the start of Section 3. He used three basic definitions of standardizing yields,

(i) yield of the best sole genotype
(ii) average yield of all sole genotypes
(iii) yield of each component genotype,

each with

(a) separate standardization in each block,
(b) same standardization for all blocks.

Some of his results for the millet/sorghum experiment are presented in Table 2: the Residual SSs for the six analyses are compared and related to the corresponding total within-block SSs, and the standard errors of a difference between two treatment means are compared and related to the overall mean. The comparison of different standardization methods is not simple. Using the best sole genotype (i) will obviously give smaller mean values and smaller variation than using the average sole genotype (ii), but, because the LER is the sum of two ratios, simply replacing the SE by SE/Mean does not fully correct for the difference in scale. The Residual SS is naturally expected to be a larger proportion of the total variation when differences between genotypes are being eliminated (iii) than when such differences are not eliminated ((i) or (ii)). Nevertheless some broad conclusions emerge from Pantelides' study: precision of treatment comparisons is improved by using a single measure of sole-crop yield ((i) or (ii)) instead of individual genotype sole-crop yields, there seems to be a small improvement in precision using the best sole genotype

TABLE 2
Comparison of results for different standardization methods for LERs

Method	*Residual* SS(45 df)	*Total within Block* SS(60 df)	*Residual/ Total*	*Standard error of difference of two means*	*Overall mean*	*SE/ Mean*
(i) (a)	0·76	2·25	0·34	0·092	0·82	0·112
(ii) (a)	1·61	4·36	0·37	0·134	1·14	0·118
(iii) (a)	8·29	10·37	0·80	0·303	1·23	0·246
(i) (b)	0·73	2·25	0·32	0·090	0·82	0·110
(ii) (b)	1·40	4·14	0·34	0·124	1·12	0·111
(iii) (b)	1·74	2·45	0·71	0·139	1·13	0·123

instead of the average, and the evidence of whether it is better to standardize each block separately is inconclusive, the difference between the two methods ((a) and (b)) being small for both experiments.

Clearly, more statistical work is needed on the choice of standardization method. N. M. Fisher (personal communication) has investigated the distribution of the LER for intercrops in Nigeria and found consistent differences between the mean LERs calculated using experiment-wise standardization and those calculated using block-wise standardization. Another advantage of block-wise standardization, which Fisher points out, is that it can provide the mean LERs with standard errors that are not obviously available from an experiment-wise standardization. However, this applies only to standard errors of a single mean LER, standard errors of differences of mean LERs being derivable directly for both standardization methods.

3.3. *The Validity of* LER *Comparisons of Different Intercrop Mixtures*

A second difficulty in comparing LERs, which is distinct from the standardization problem, is discussed by Mead and Stern (1979) and Mead and Willey (1980). The use of the LER to measure the yield advantage available to the farmer rests on the implicit assumption that the yield proportions obtained from a crop mixture are exactly those required by the farmer. If two LERs are to be compared, these will usually have different yield proportions and will therefore use different definitions of biological efficiency.

For example, in an experiment at ICRISAT on maize and pigeonpea the results for the two "best" mixtures, in kg/ha, were

Mixture I		*Mixture* II	
Maize yield	2234	*Maize yield*	3130
Pigeonpea yield	896	*Pigeonpea yield*	571

The optimal sole-crop yields were 3400 and 1035 kg/ha for maize and pigeonpea respectively. The calculations for the LERs are as follows:

		Mixture I	*Mixture* II
Standardized maize yield	M_A	0·66	0·92
Standardized pigeonpea yield	M_B	0·87	0·55
	LER	1·53	1·47
maize/(maize and pigeonpea) proportion		0·43	0·63

It may be misleading to argue that mixture I is better than mixture II because of the larger LER. If the desired proportion of maize in the mixture were 0·6, mixture II might well be preferred. Thus, while an interpretation of any single LER can easily be given, a comparison of two LERs is not necessarily sensible.

A general method of obtaining the "Effective" LER for any predetermined crop proportion was given by Mead and Stern (1979), and, in more detail, by Mead and Willey (1980). Suppose that a proportion, k, of the area is sown with the intercrop and a proportion $(1-k)$ with sole crop A. Consider standardized yields for each crop, so that all yields are proportions of the best possible sole-crop yield. Let the standardized yields for the mixture be M_A and M_B and for the sole crops of the two components of that mixture, S_A and S_B. Then the total standardized yield is

$$k(M_A+M_B)+(1-k)S_A,$$

which, if $k = 1$, becomes the usual LER. The proportion of crop A of the total harvest is

$$\lambda = \frac{kM_A+(1-k)S_A}{k(M_A+M_B)+(1-k)S_A} \quad \text{if } \lambda > \lambda_{\max} = \frac{M_A}{M_A+M_B}.$$

To achieve a required proportion λ (greater than λ_{max}), we must have

$$k = \frac{S_A(1-\lambda)}{\lambda M_B - (1-\lambda) M_A + (1-\lambda) S_A}.$$

The total standardized yield is then

$$\text{LER}_\lambda = \frac{M_B S_A}{(S_A - M_A) + (M_A + M_B - S_A)\lambda}, \quad \lambda > \lambda_{max}. \tag{1}$$

For $\lambda < \lambda_{max}$, LER_λ is the same except that A and B are reversed. Hence for any desired ratio of crop A to the total crop, the biological efficiency of the system can be calculated. These calculations require sole-crop yields though these need not be obtained from experimental plot yields (see Section 2.3).

To illustrate the use of the effective LER we return to the millet/sorghum genotype data described at the start of Section 3. The LERs are in Table 3 together with the millet and sorghum mean yields standardized with respect to the best sole-plot mean yield for each crop. With the sole-crop mean yields for millet genotype 3 and sorghum genotype 4 as the standardizing yields, the simple LER is plotted against the harvested proportion of millet in Fig. 7. Clearly, measured against the severe requirement of performing better than the combination of the best genotypes of each crop, most intercrops do not perform well. This is emphasized in Fig. 8 where effective LER is plotted against required proportion of millet, λ, for each of the sixteen genotype combinations. Each curve reaches a peak LER where $\lambda = \lambda_{max}$ for that curve.

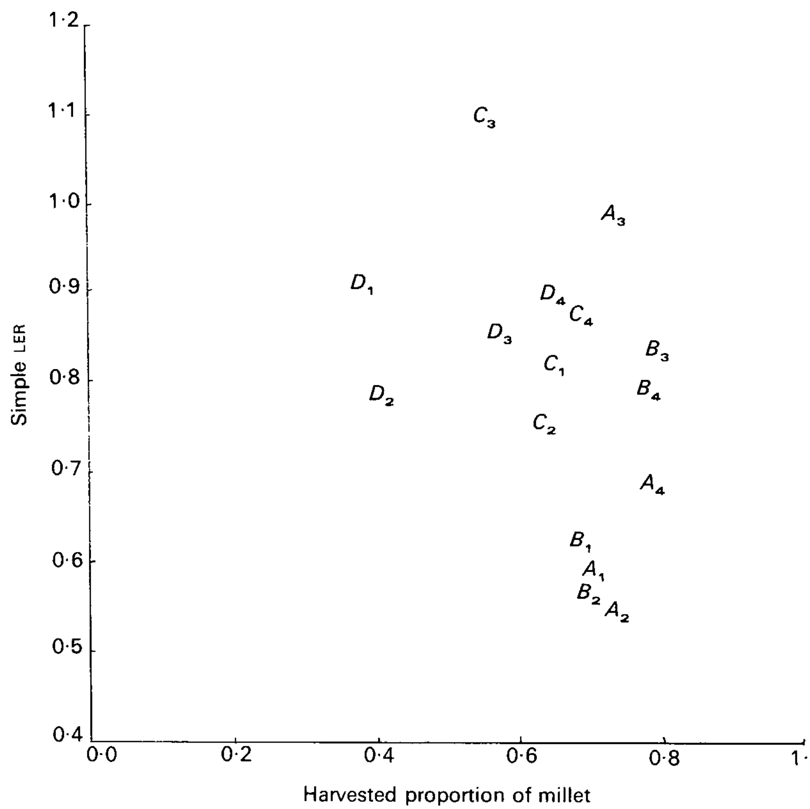

FIG. 7. Simple LER plotted against harvested proportion of millet $A_1 = M_1 S_1$, $A_2 = M_1 S_2$, $A_3 = M_1 S_3$, $A_4 = M_1 S_4$, $B_1 = M_2 S_1$, $B_2 = M_2 S_2$, $B_3 = M_2 S_3$, $B_4 = M_2 S_4$, $C_1 = M_3 S_1$, $C_2 = M_3 S_2$, $C_3 = M_3 S_3$, $C_4 = M_3 S_4$, $D_1 = M_4 S_1$, $D_2 = M_4 S_2$, $D_3 = M_4 S_3$, $D_4 = M_4 S_4$.

TABLE 3

Effective LERs *for the sixteen millet/sorghum combinations*

	millet yields kg/plot	sorghum yields kg/plot	standardized yields S_M	S_S	M_M	M_S	$\lambda_{max} = \frac{M_M}{M_M+M_S}$	LER
*M*1*S*1	2·025	1·430	0·62	0·69	0·44	0·18	0·710	0·62
*M*2*S*1	1·975	1·215	0·51	0·69	0·43	0·15	0·741	0·58
*M*3*S*1	3·462	2·237	1·00	0·69	0·75	0·28	0·728	1·03
*M*4*S*1	2·650	1·182	0·70	0·69	0·58	0·15	0·794	0·73
*M*1*S*2	2·062	1·572	0·62	0·47	0·45	0·19	0·703	0·64
*M*2*S*2	1·950	1·407	0·51	0·47	0·42	0·17	0·711	0·59
*M*3*S*2	3·257	1·412	1·00	0·47	0·71	0·17	0·807	0·88
*M*4*S*2	3·007	1·480	0·70	0·47	0·66	0·18	0·786	0·84
*M*1*S*3	2·252	2·417	0·62	0·98	0·49	0·30	0·620	0·79
*M*2*S*3	2·332	2·280	0·51	0·98	0·51	0·28	0·646	0·79
*M*3*S*3	2·942	4·012	1·00	0·98	0·64	0·50	0·561	1·14
*M*4*S*3	2·902	2·377	0·70	0·98	0·63	0·29	0·685	0·92
*M*1*S*4	1·647	4·660	0·62	1·00	0·36	0·58	0·383	0·94
*M*2*S*4	1·567	3·775	0·51	1·00	0·34	0·47	0·420	0·81
*M*3*S*4	2·387	3·005	1·00	1·00	0·52	0·37	0·584	0·89
*M*4*S*4	2·832	2·555	0·70	1·00	0·62	0·32	0·660	0·94

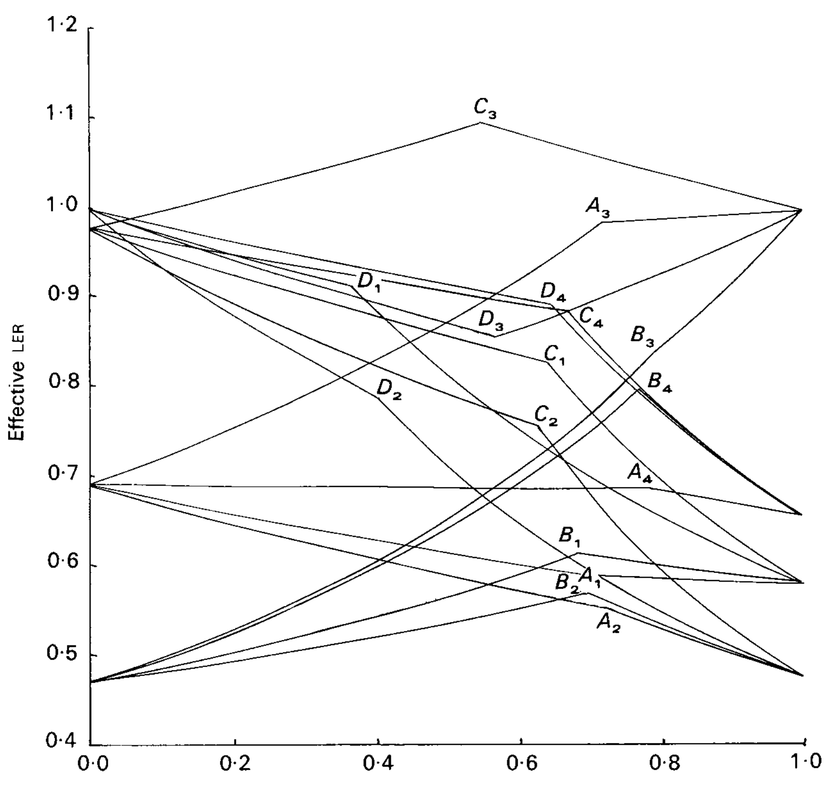

FIG. 8. Effective LER plotted against λ, standardization using best genotype. Intercrop combinations as in Fig. 7.

Many agronomists would argue, however, that the interesting measure of intercrop efficiency should be based on standardization by the mean yields for the genotypes actually included in the intercrop mixture. Fig. 9 shows the effective LER curves for this method (essentially M_A, M_B are divided by S_A, S_B and S_A and S_B are set equal to 1 in (1)). Almost all the intercrops now show advantage over the corresponding sole crops. (Note that the effective LER inevitably tends to 1 as λ tends to 0 or 1). With this standardization the comparison of effective LERs for different combinations is less simply interpreted, though the concept of one mixture giving greater biological efficiency than another for a particular range of λ values is still valid.

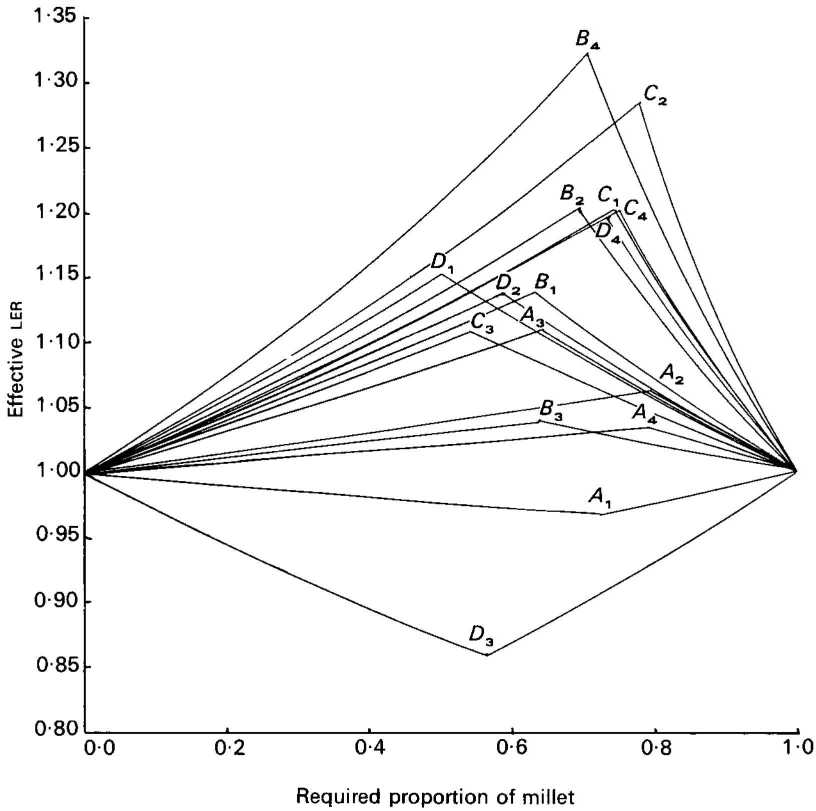

FIG. 9. Effective LER plotted against λ, standardization using different genotypes. Intercrop combinations as in Fig. 7.

The millet/sorghum genotype data have been used throughout this paper because they illustrate well the methods discussed. However, it is not a common intercrop mixture and may give a rather misleading impression of the advantages of intercropping. We therefore include a final example of effective LERs for the sorghum/pigeonpea intercrop, which is very common in India. The data came from the experiment, discussed in Section 2.4, to compare 17 pigeonpea genotypes using a single sorghum genotype. Using standardization by the best pigeonpea genotype mean yield gives the effective LER curves shown in Fig. 10. One of the concepts promoted by Mead and Willey (1980) is that of an "envelope" of effective LER curves which may indicate limits to the intercrop advantage; the way in which the right-hand halves of many of the curves in Fig. 10 bunch closely together could be used to support this concept.

The effective LER indicates clearly those combinations giving the highest yield advantages for a given required proportion of crops but precise comparisons cannot yet be made because the underlying sampling distribution of the LERs is not known. This disadvantage does not occur

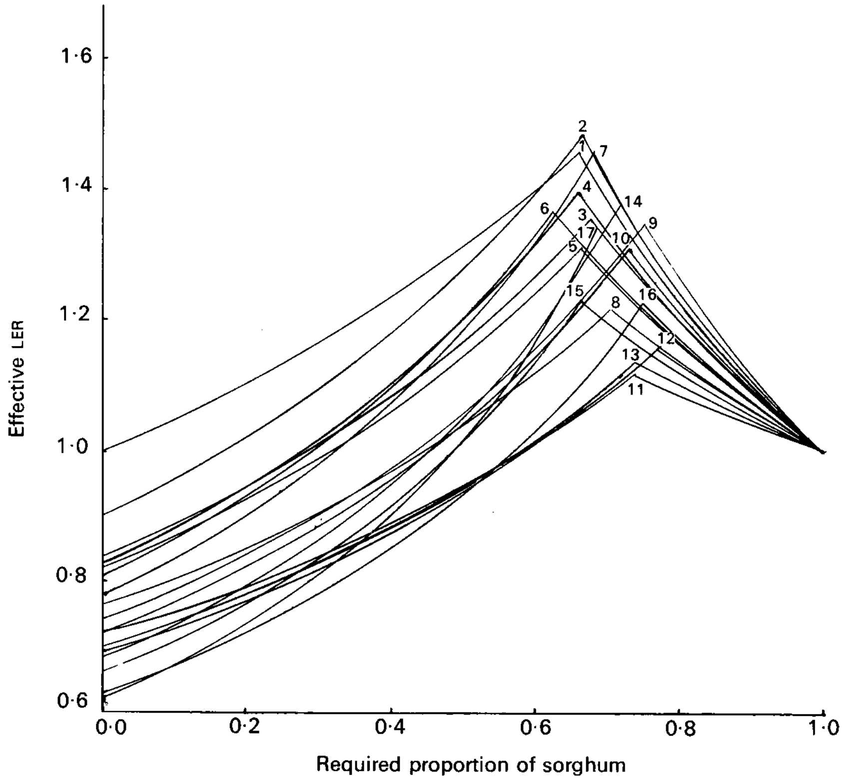

FIG. 10. Envelope of LER curves for sorghum/pigeonpea mixtures.

with the bivariate method where exact tests are available for comparisons between means. However, unlike the bivariate method, the LER is easy to interpret, in terms of practical advantage, and comparisons can be made between sole-crop and intercrop yields.

3.4. *The Bivariate Method*

Use of a single bivariate analysis for the yields x_1 and x_2 of the two crops was proposed by Pearce and Gilliver (1978, 1979) following an idea of Steel (1955). Pearce and Gilliver gave two different, althought equivalent, methods for dealing with the two yields and their possible correlation. In the first approach, the error variances of x_1 and x_2 are V_{11} and V_{22} and their error covariance V_{12}. After each variate has been adjusted by the other (as in covariance), the variances become V'_{11} and V'_{22} where

$$V'_{11} = V_{11} - V_{12}^2/V_{22} \quad \text{and} \quad V'_{22} = V_{22} - V_{12}^2/V_{11}.$$

Two new variates, y_1 and y_2, which can be plotted with rectangular axes, are defined as

$$y_1 = x_1/\sqrt{V_{11}} \quad \text{and} \quad y_2 = (x_2 - V_{12}\, x_1/V_{11})/\sqrt{V'_{22}}$$

having error variances equal to1 and error covariance equal to 0, i.e. y_1 and y_2 are independent.

In the second approach, the correlation between x_1 and x_2 is allowed for by skew axes. The transformed yields are now

$$z_1 = x_1/\sqrt{V'_{11}} \quad \text{and} \quad z_2 = x_2/\sqrt{V'_{22}},$$

the cosine of the angle θ between the z_1 and z_2 axes being $V_{12}/\sqrt{(V_{11}\, V_{22})}$, i.e. the correlation coefficient between x_1 and x_2. As Fig. 11 shows, the same point M, representing the same combined yield, is achieved by either method (since $PQ = V_{12}\, x_1/V_{11}\sqrt{V'_{22}}$).

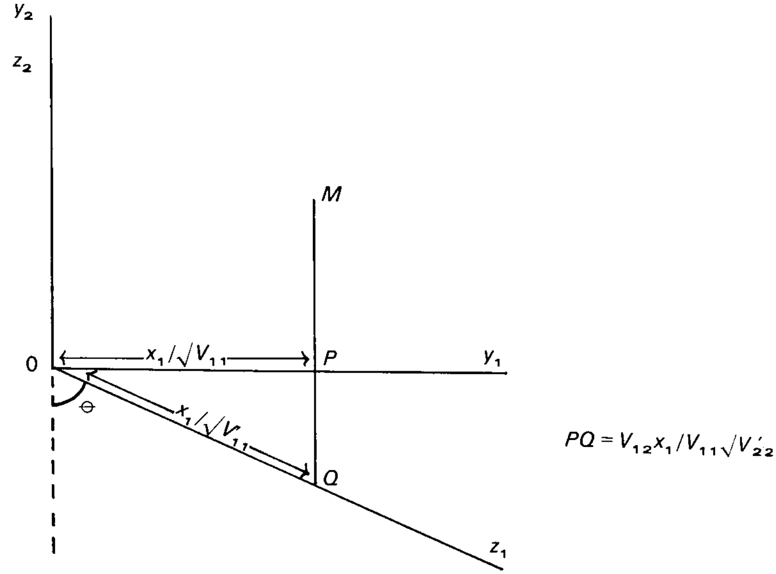

FIG. 11. Graphical presentation of yields of two crops.

Since y_1 and y_2 are uncorrelated and their common variance is 1, a circle of radius $1/\sqrt{n}$ indicates a standard error of a mean of n observations. The transformed yields z_1 and z_2 are easier to visualize than y_1 and y_2. For a positive correlation between x_1 and x_2, as z_1 increases, z_2 has to increase to achieve the same value of x_2. Similar results hold for a negative correlation between x_1 and x_2 when the skew axis would lie above the horizontal. Provided x_1 and x_2 are obtained from mixed plots, standard multivariate techniques can be used to test the significance of differences between treatments.

The data from the sixty-four millet/sorghum mixed plots were analysed using the bivariate method. The sums of squares and products for the yields of millet (x_1) and sorghum (x_2) are in Table 4. The residual variances of x_1 and x_2 are $V_{11} = 0{\cdot}307$ and $V_{22} = 0{\cdot}265$ and their residual covariance $V_{12} = -0{\cdot}046$. The variances after adjusting each variate by the other are $V'_{11} = 0{\cdot}299$ and $V'_{22} = 0{\cdot}258$. The two new variates $y_1 = 1{\cdot}804x_1$ and $y_2 = 0{\cdot}297x_1 + 1{\cdot}969x_2$ have residual variances of 1 and residual covariance 0. The sums of squares and products for y_1 and y_2 are in Table 5.

In the second approach, the transformed yields are $z_1 = 1{\cdot}828x_1$ and $z_2 = 1{\cdot}969x_2$, the angle θ between the z_1 and the z_2 axes being $\cos^{-1}$ (0·161).

Fig. 12 shows the sixteen mean yields plotted after transformation to the y_1 and y_2 scales and the sole-sorghum and sole-millet mean yields after transformation to the z_1 and z_2 scales. The skew axis z_1 forms an angle of 9·34° with the horizontal, indicating a negative correlation

TABLE 4
Bivariate analysis of variance for yields of millet and sorghum (kg/plot)

source	*df*	$ss(x_1)$	$sp(x_1 x_2)$	$ss(x_2)$	r
blocks	3	0·33	0·67	1·76	
millet genotypes	3	14·78	0·18	5·76	0·02
sorghum genotypes	3	2·58	−7·93	47·44	−0·71
interaction	9	2·26	−0·78	15·71	−0·13
residual	45	13·83	−2·09	11·92	−0·16
total	63	33·78	−9·95	82·59	

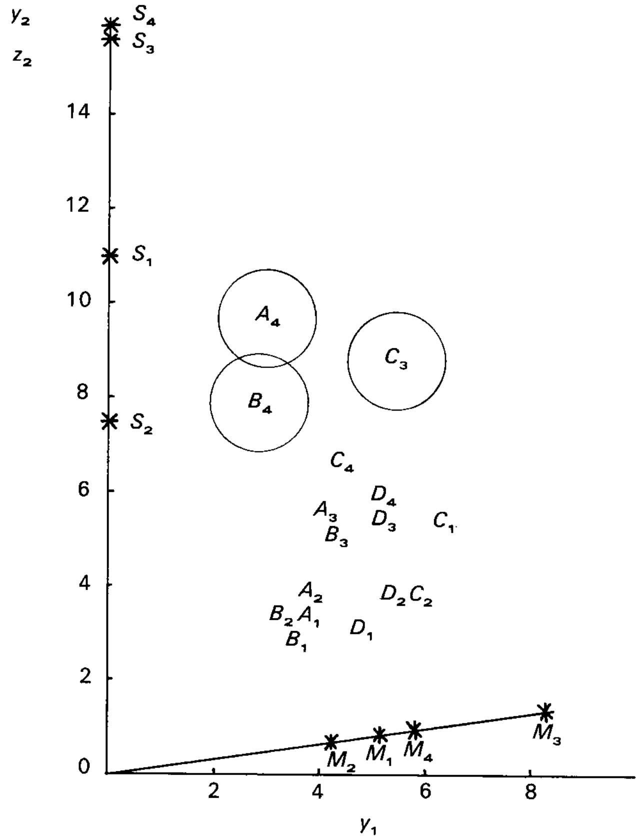

FIG. 12. Graphical presentation of mixed-crop yields and sole-crop yields for millet/sorghum experiment. Intercrop combinations as in Fig. 7.

TABLE 5
Bivariate analysis of variance for transformed yields (kg/plot)

source	*df*	$ss(y_1)$	$sp(y_1\, y_2)$	$ss(y_2)$
blocks	3	1·08	2·56	7·65
millet genotypes	3	48·10	8·56	23·87
sorghum genotypes	3	8·41	−26·77	174·90
interaction	9	7·36	−1·56	60·20
residual	45	45·00	0·00	45·00
total	63	109·95	−17·21	311·62

between the species. Each intercrop mean represents the average of four observations and has a standard error of $1/\sqrt{4} = 0{\cdot}5$. The circles of radius 1 indicate 95 per cent confidence regions and are given for three of the points.

The differences between the points in Fig. 12 are also apparent from Table 5, which shows that the mean squares for the main effects and interactions of y_1 and y_2 are all considerably largé. These effects can be seen to a lesser extent by considering the x_1, x_2 table of sums of squares and products (Table 4).

The use of bivariate analysis for intercropping cannot be properly assessed until research workers have used the methods extensively. A disadvantage of the method is that, although the use of graphical plots is central to the analysis, the statistical technique emphasised is that of significance testing which experimenters are only too willing to use at the expense of estimation. Another difficulty is that comparisons between sole and intercrop yields are not easily made.

A fundamental assumption of the method is that the correlation between the yields for the two crops is constant for all treatments. This assumption is difficult to check because replications for each treatment are usually few. Pantelides (1979) applied a test derived by Rao (1965, section 6g.4) to the millet/sorghum data just discussed, and although the correlations for individual treatments varied from $-0{\cdot}97$ to $0{\cdot}85$ the χ^2 statistic suggested by Rao showed no significant variation of correlation!

The correlation on which the skew axis diagram depends is based on the within-treatment correlation. The plotting of treatment means in the diagram essentially displays the between-treatment correlation against a background of the within-treatment correlation and it may be expected that this display will be most clear when the between-treatment correlation is very different from the within-treatment correlation. The pattern of correlations for the millet/sorghum data can be seen in Table 4 where the correlations for individual lines of the bivariate analysis of variance are considered. The value of $-0{\cdot}71$ is clearly different from those for the rest of the experiment and a presentation in the form of Table 4 could provide a useful summary of the results before proceeding with the bivariate method.

To obtain as much information as possible about the results of a mixed-cropping experiment it seems sensible to use both the LER and the bivariate methods. This enables accurate comparisons between mean yields to be made while giving a clear diagrammatic presentation of the effective LERs for given desired proportions of intercropping. There is a need for further investigation of the relationship between effective LERs and the bivariate method; simple LERS can be represented on the bivariate diagram by ratios of distances.

3.5. *Stability*

The farmer's preference for intercropping implies that this farming system provides him with sufficient yields even in adverse conditions. Experimental evidence confirms that, even if pests, disease or poor weather destroy or inhibit the growth of one of the component crops, the second crop provides a yield which, in sole cropping, would not be achieved (Section 1.3). Much of the advantage of intercropping, and in particular the possible stability of yield, can be attributed to the use of different niches, of either time or environment, by the two crops. The literature on stability is not extensive; a review was given by Trenbath (1974).

Daniel (1955) showed that, although in any given year a mixture of barley and oats sometimes yielded less than the better of the two sole crops, the difficulty in predicting which sole crop would be the better meant that the highest "expected" yield was achieved with the mixture. Maximizing expected total yield is one approach to stability. Other approaches have included minimizing variation of various yield variables. However, the two principal approaches to assessing stability have been (1) the use of measures introduced for genotype-environment interaction studies, and (2) in terms of risk.

The method of measuring stability of genotypes by regressing yield of a particular genotype on the mean yield of a group of genotypes over a range of environments was initiated by Yates and Cochran (1938) and revived by Finlay and Wilkinson (1963), and reviewed by Freeman (1973). Recently it has been tried for intercropping when, instead of different genotypes, three cropping systems (sole A, sole B and mixed) are used (Rao and Willey, 1980; Mercer-Quarshie, 1979). As the dependent variable in the regression is included as one component of the independent variable there are plainly difficulties when, as in these papers, the independent variable is the average of only three yields.

Risk to the farmer has been considered by several authors, notably Francis and Sanders (1978), Gahlot *et al.* (1978) and Spurling (1973). Rao and Willey (1980) examined data from 89 experiments of sorghum/pigeonpea intercrops and, using the market prices charged at the time of writing, calculated the probability of returns falling below a reasonable subsistence level. Their results showed a risk of one in eight years for sole sorghum, one in five for sole pigeonpea, one in thirteen for a shared crop (half area sole sorghum, half sole pigeonpea) and one in thirty-six for the intercrop. Disadvantages of this approach are that crop prices fluctuate greatly and that the farmer does not necessarily require recommendations made on a monetary basis; his main aim may be to provide sufficient yields of each crop for his family's needs, not to gain the greatest cash profit.

Further research into measurement of stability, both over locations and over times, is obviously required when data are available from multi-locational experiments and when comparisons can be made for experiments over years. However, the biggest difficulty in discussing stability remains the lack of any clear definition of an index of stability of yield. With large variations between environments there seems no prospect of fitting yield data by simple statistical distributions from which a particular parameter could represent stability, and there remains a need for a sensible index that can be estimated statistically. We believe that the most promising approach is that of minimizing the risk to the farmer since this philosophy is clearly of practical relevance, but the detailed definition of risk for multiple crops needs much careful thought.

4. CONCLUSIONS

We believe that intercropping will be a major focus of agricultural research for the next decade and that very large resources will be devoted to experiments on intercropping systems. This offers a considerable challenge to statisticians both to show how the advances in statistical knowledge of experimental design and analysis can be used to improve the efficiency of intercropping experiments, and to develop new methods for the special requirements of experiments with more than one crop.

The topics for which knowledge exists and on which experimenters most need advice are:

(i) The importance of factorial structure in experimental design to give improved efficiency;
(ii) The disadvantages of split-plot designs when not necessary from practical considerations;
(iii) the need for several different analyses for each set of data from an experiment including both mixed and sole cropping; and
(iv) the use of cheap modern computing facilities to achieve these analyses.

Areas where statistical research seems most to be needed by experimenters are:

(a) The problems of analysing combined yield indices, in particular the properties of analyses using various different standardization methods and modifications of the Land Equivalent Ratio index;
(b) the properties of bivariate analysis for intercrop yields, including the analysis of correlations and the estimation of effects involving both mixed and sole crops;
(c) development of designs for investigating the effects of spatial arrangement factors for intercrops, and the assessment of the practical and theoretical advantages and disadvantages of systematic designs; and
(d) methods for measuring and analysing stability.

This last is perhaps the area which the research workers involved in intercropping would identify as the least understood.

We hope that this paper will stimulate interest and activity, both in discussion of what we see as the important statistical ideas in intercropping, but more importantly in solving the practical and theoretical problems and in promoting better research.

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DISCUSSION OF THE PAPER BY MR MEAD AND MISS RILEY

Professor S. C. PEARCE (University of Kent at Canterbury): The paper we have heard this evening is both comprehensive and topical and I have pleasure in proposing the vote of thanks. With intercropping statisticians are faced with quite a new set of problems and it is good that we should try to come to terms with them. Our techniques were developed in the temperate zone where sole crops are the rule. We clear a parcel of land and plant it up for a single species, though allotment holders and fruit growers may occasionally do something else. In the past most agricultural research in the tropics has been on cash crops, like rubber and tea, but the pressure of world needs directs us to research on food crops as well and here we have to work in a context of intercropping. Peasant farmers grow their crops in intimate association, so that land, water and fertilizer are exploited to the full. Further, if one crop fails another can take over. The speakers place some emphasis on the semi-arid tropics, but I suggest that intercropping is of importance in the humid tropics as well.

I cannot pretend to agree with them at every point. On factorial design and confounding they first tell us that one advantage of factorial design lies in the hidden replication it affords, but that is so only if there